

13858/P

THE

PERIODICAL MATURATION AND EXTRUSION OF OVA,
INDEPENDENTLY OF COITUS,

IN

MAMMALIA AND MAN,

PROVED TO BE

THE PRIMARY CONDITION TO THEIR PROPAGATION.

~~~~~  
Translated from the German

OF

TH. L. W. BISCHOFF, M.D. & PHIL. D.

PROFESSOR OF MEDICINE, AND DIRECTOR OF THE PHYSIOLOGICAL INSTITUTE  
AT GIESSEN, ETC.

BY

HENRY SMITH,

FELLOW OF THE ROYAL COLLEGE OF SURGEONS; AND SURGEON TO THE  
ROYAL GENERAL DISPENSARY.

=====

LONDON:

PRINTED BY WILSON AND OGILVY, 57, SKINNER STREET.

~~~~~  
1845.



(*From the London Medical Gazette.*)

WHOEVER occupies himself with the history of the various theories of generation, must quickly perceive that most of the errors in that department are, and have been, without doubt, referrible to a want of acquaintance with the primary formative material for the future being, the ovum, and its pre-existence in the ovary, entirely independent of coitus in mammalia and man. This necessarily induced a distinction and a gap of separation between mammalia and man on the one side, and the rest of the animal kingdom on the other, which hiatus, indeed, many observers endeavoured to repair and fill up; but the importance and necessity of objective information presented themselves with such force, that for this very reason the greatest and most illustrious authorities remained in doubt and error. There are probably few subjects which prove the dependence of human knowledge upon objective investigation in a more striking manner, than that which is now in question. Philosophers, theologians, physicians, and naturalists of every country, have laboured to overleap this little deficiency of the objective proof of the ovum in mammalia and man, and all their speculative cogitation, all their combinations and theories, led them into obscurity. All the world became firmly convinced that mammalia and man formed an exception to the mode of development of the rest of organized beings, and to the conditions to which they are subject.

We observe universally with respect to other animals and to plants, that, with proportionately very few exceptions, their propagation is so conditioned, that the older primitive organisms produce certain materials, ova and fecundating material, from the reciprocal influence of which upon each other, the germ, endued with the capacity of development, (*entwicklungsfähig*) results. At the same time, we further observe, as an universal rule, that the formation and coming into contact of these two generative materials, however necessary both may be for the maintenance of the species, are nevertheless quite independent of, and, in reference to each other, for the most part quite accidental. The ova form, and become mature, and are extruded from the maternal organism, usually at fixed periods, having regularly recurring intervals, quite independently of the formation and maturation of the fecundating material in the male, which takes place either in like manner periodically or even continuously. By the operation of accessory circumstances dependent on external causes,—and which may either arise absolutely from without, and be entirely accidental, or may be caused by certain vital phenomena simultaneously developed in some other manner,—both materials are brought into connexion with each other, and thereby the germs rendered capable of

development. Should these circumstances not come into operation, or should an interruption develop itself in their course, the generative materials mature notwithstanding, and are separated, although no germs capable of development result from them. Examples of this in the vegetable kingdom, and in the lower classes of animals, as well as in fishes, amphibia, and birds, are too well known to require mention here.

On the other hand, in reference to mammalia and man, the case stood quite different. With respect to them, the formation of a germ was quite universally regarded as the result of coition; that act, in their case, being considered not only to have the design of rendering the female generative matter capable of development, but also of altogether producing it for the first time (creating it, *erst hervorbringen*).

This opinion was of course chiefly based upon ignorance of the existence of the female generative material, the ovum, previously to coition.

So stood the matter, when, at length, after centuries of controversy, Von Baer, in 1827, discovered the ovarian ovum of mammalia and man, and at the same time, in its unexpected minuteness, the cause of its long concealment. I have always felt astonished that this discovery, in a department of science which has interested mankind in all ages in so extraordinary a degree, did not excite greater and more universal attention. By some it has been received, by some denied, taken no cognizance of by others; and only embryologists, in the strictest sense of the word, have occupied themselves with it, and they indeed only in its relation to the development of the embryo, and not with reference to the theory of generation in particular. This theory, which had so often been erected upon an hypothetical ovum, was so firmly based as even to suppress for a time the necessity for investigation as to how it might harmonize with what was founded on fact, after that very matter of fact became known. Although the existence of the ovum previous to, and independently of, all coition, was proved, yet coition was still held to be the sole and necessary condition to the maturation and extrusion of an ovum from the ovary; and all other circumstances, however distinctly they might indicate the contrary, were considered only from that point of view.

I was myself so much influenced by this theory, as to be led blindly by it in the course of my previous researches on the development of mammalia. They were, it is true, directed more particularly to the development of the fecundated ovum; but the process of fecundation was an essential object of them, and as I availed myself of the knowledge of the ovum I certainly succeeded in rectifying and throwing light upon several points which my predecessors had remained unacquainted with. Yet still I was always biassed by the opinion, that the passing out of the ovum from the ovary, the first condition therefore of its development, must have in some way or other a necessary relation to copulation. Like all who had preceded me,

I sought only to ascertain at what period the ovum freed itself from the ovary after the first act of coition in animals, and what was the part performed at the same time by the seminal fluid. I was so fortunate, although in this track, as to decide several of the most important questions; I furnished proof, that the male fluid comes into material contact with the ovum, and found by indubitable observations that the seminal fluid penetrates through the uterus and fallopian tube up to the ovary. Accordingly, in the sense of the old doctrine, I laid down the proposition, that the fecundation of the ovum of mammalia takes place upon the ovary, and in various kinds of animals at various periods after copulation, within which periods the seminal fluid advances up to the ovary, and the ova are extruded from the ovary.

Meanwhile, from continued observations and researches, I have now arrived at the conviction, that although the facts upon which I based that proposition are still perfectly correct, yet that nevertheless it by no means comprehends the law of the generation of mammalia and man, but that this latter is more comprehensive, and completely accords with that which governs the generation of all organised beings. It is as follows:—

“Both in mammalia and man, the self-forming ova in the ovaries of the female individuals, undergo a periodical maturation quite independently of the influence of the male seminal fluid. At this period, which in animals is usually called ‘the heat,’ in the human female, ‘menstruation,’ these mature ova disengage themselves from the ovary, and are extruded. At such time, also, the sexual impulse manifests itself, in the female animal only, ^{chiefly at this time} ~~more particularly~~ in woman. If copulation take place, the fecundation of the ovum is effected by the material influence of the semen upon it. If copulation do not take place, the ovum is nevertheless extruded from the ovary, and enters the fallopian tube, but there proves abortive, (zu grunde geht). The relations in respect to time in this matter may vary, although, as it appears, to a different, but yet definite extent in different animals. The seminal fluid may have sufficient time to reach the ovary before the ovum leaves it. The ovum may have already quitted it, and the seminal fluid first come in contact with it in the fallopian tube. The influence of the semen must, however, always be exercised within the tube, in order to produce development of the ovum, which, indeed, first commences its evolution within that duct. But only at this season of the periodical maturation of the ova can coition have fecundation for its result.”

I do not consider it necessary here to adduce proof of every element comprised in this law, inasmuch as many of them stand as already settled and familiar truths; but I shall in the first place restrict myself especially to proving, that at the time of heat, the ova in mammalia quit the ovary, and reach the fallopian tube, whether copulation take place or not, and

whether the semen be conducted to the ovum or not. Previously, however, I would add the following observations.

The changes in the female genital organs of mammalia at the time of heat, which demonstrate their periodically increased activity at that season, were long since partially known. I may, however, mention in addition, that Barry and I both have drawn attention also to the changes in the ovum observable at this period, and which may, therefore, be regarded as signs of its maturity. They relate in the first place to its size. The most mature ova are at the same time always the largest, which fact meanwhile bears out the law already announced by Von Baer and Valentin, and confirmed by me, in reference to the individual parts of the ovum, and the Graafian vesicle, viz. that the contained parts are always relatively smaller than the containing, in proportion as the parts are more mature. The vitellus is most full and most dense, and contains most vitelline particles, in mature ova. The number of the larger fat globules contained in it, which varies much in different animals, appears to decrease; and on the contrary, that of the smaller vitelline granules to increase. The germinal vesicle, which, in the immature, is situated more in the centre of the yolk, is, in the mature ovum, found quite at the circumference, and sometimes becomes visible there, as though placed in a notch of the yolk: I have seen this in dogs. It may even be wanting in perfectly mature ova, and according to some observations made in dogs, it seemed possible that the disappearance of the nucleus of the germinal cell taking place previous to that of the cell itself, denoted the complete maturity of the ovum. The most striking and most easily recognisable, at the same time a sure sign of the full maturity of the ovum; in dogs and rabbits at least, is afforded by the change undergone by the cells of the so-called discus proligerus, round about the zona, which commence to extend themselves into fibres, and being placed upon the zona in this form give a radiated appearance to the entire ovum. Lastly, I can decidedly assert, with respect to the dog, that the formation of the corpus luteum in the form of granulation-like excrescences (*den granulationen ähnliche Wucherungen*) from the inner surface of the Graafian vesicle, commences previous to the opening of the latter, and the extrusion of the ovum, and may, therefore, also be considered as a sign of perfect maturity. These statements may, I hope, serve to guide others, in deciding the question in any given instance, as to whether they have mature ova, or such as are advancing to maturity, before them.

A repetition of the experiments made by Nuck, Haighton, Cruikshank, Grossmeyer, Blundell, and Hausmann, first, however, convinced me, that it is not the influence of the semen which causes the extrusion of the matured ova from the ovary: it may not be altogether unnecessary to quote and critically examine the results of those experiments in the next place.

Nuck (*Adenographia curiosa*, p. 69, op. omn. Lugd. Bat. 1773) put a liga-

ture round the left cornu of the uterus of a bitch three days after coitus, and found, twenty-one days after, two ova in the portion of the uterus above the ligature, but none below it. Although this result is rendered very dubious by his addition, "*foetus jam consumptos et in materiam pene purulentam conversos fuisse*,"—although it is further certain that on the twenty-first day after the first coitus the embryo of the dog is not yet formed, or scarcely in its first rudiments,—yet still, if received to be correct, there is nothing in it to excite astonishment. The semen of the dog penetrates in copulation at once up into the point of the uterus; long before the third day after coitus it has entered the fallopian tubes; the ova also have already left the ovary and become fecundated, so that the tying of the uterus would only prevent the sinking down of the ova below the point of ligature: all the rest is readily explainable.

Haighton (Reil's Archiv. III. p. 46), divided the fallopian tube in a great many rabbits, both with and without a loss of substance, and sometimes on one, sometimes on both sides. In by far the greater number of instances he remarked a complete loss of sexual impulse afterwards: (probably because the vessels leading to the ovaries were also divided, whereby the maturation and development of the ova, and with that also "the heat" and sexual impulse, were arrested). When even the act of copulation was permitted by some of them, it proved ineffectual, and the ovaries had in most instances degenerated. Coitus and conception, however, took place in three instances in which he had divided the fallopian tube on one side only. He thence concludes, that the ovaries might become affected by the stimulus of impregnation, without contact with the seminal fluid, whilst he should have been content to rest on the assertion that the ova quit the ovary, even when the semen cannot reach the latter. In one rabbit, in which he had divided the fallopian tube on one side six hours after coition, he subsequently found corpora lutea on both sides, but ova on the uninjured side only. This may also be easily explained; indeed, in two ways. In the first place, Barry and I have seen the semen in the rabbit upon the ovary within nine and ten hours after coition. It is, therefore, quite possible that within six hours it might have already passed the point of ligature. The ova consequently might have been fecundated, and a tubal gestation have been produced. If, however, this view be not received, then, in the second place, the mature ova may have been extruded on that side on which the semen could not reach them, but there proved abortive because unfecundated, while at the same time corpora lutea were formed just as well as on the opposite side.

The experiments of Grassmeyer (*De Fecundatione et Conceptione Human.* Dissert. Götting. 1789, p. 48) are less to be relied on, and no conclusion can be drawn from them. He placed a ligature round the fallopian tube, or the uterus, in fourteen rabbits; after which simple

operation only two survived,—a proof that the operation was badly conducted. One of these two animals, in which he had tied the fallopian tube, was with young at the time of the operation, and in consequence aborted. Four days afterwards sexual impulse was ardently exhibited, and the act of coition consummated. On killing the animal fourteen days later, he found the cornua of the uterus in some parts somewhat swollen, but no ovum and embryo, and likewise nothing worthy of remark in the ovaries. In the abdominal cavity, however, he found hydatids, which he looked upon as something very extraordinary, although Blumenbach explained their nature to him. In the other rabbit the fallopian tube was also tied. It is represented as having become impregnated twenty-one days afterwards, although Grassmeyer did not see the act of coition; and nine days subsequently found no change whatever, either in the ovaries or in the uterus.

Cruikshank also made an experiment bearing on this point, but which was without result. He placed a ligature around the left fallopian tube of a rabbit, close to the uterus, a day after coitus. After fourteen days he found the uterus of the right side without embryo, although the placenta was perceptible, and had retrograded in the process of formation; the same was the case with the ovary. On the left side was no sign of fecundation in the uterus, no placenta, the tube very wide and soft, the ovary twice as large as that of the other side, red, and covered with coagulable lymph. In the fallopian tube was seen an hydatid, which contained a clear fluid, but no embryo. In addition to this there were general traces of peritonitis and exudation in the abdominal cavity. In this instance, according to my experience, the fecundation and extrusion of the ova must have taken place within twenty-four hours. It appears, however, that the operation had excited violent inflammation, and that from this cause the ova on both sides had aborted, though, on the right side, development had advanced somewhat farther (*Philosophical Trans.* 1797, vol. i.: Experiments 11 and 16.)—

Blundell (*Medico-Chir. Trans.* vol. x. p. 264, 1819; *Meckel's Archiv.* v. p. 422; *Principles and Practice of Obstetricity*, Lond. 1824, p. 60) found, after division of one cornu of the uterus in rabbits, prior to and after coition, ova only in the uninjured cornu, but corpora lutea, which were not distinguishable from each other, on both sides. After division of the vagina, ova were never found in the uterus, but still corpora lutea in the ovaries. At the same time he remarked an insatiable desire for copulation after these operations.

Lastly, Hausmann (*Ueber die Zeugung des wahren weiblichen Eies*, p. 93), in his 53d experiment, on a sow, in which he had removed the fimbriæ of the fallopian tubes from contact with the ovaries, saw no fecundation, but still the development of corpora lutea followed. A

second experiment (54), in which he did the same with only one ovary, is not to be relied upon.

The following are the experiments which I have myself made in reference to this point.

On the 29th of January, 1842, at half-past 8 A.M., I permitted a rabbit to receive the male; the spermatozoa were present in the vagina in large quantity. At half-past 2 P.M., after six hours therefore, I opened the animal, and cut out its right ovary and fallopian tube; in doing which, I found that this was one of those rabbits from which I had removed the uterus in the previous summer. The ovary and fallopian tube, notwithstanding, exhibited all the signs of "the heat," were injected with blood, and turgid; several Graafian vesicles were much swollen. The ciliary motion on the epithelium of the tube was very active; the tube, however, naturally enough, contained no spermatozoa. I proceeded to examine the four largest and most swollen Graafian vesicles, and found them still closed, with an ovum in each one. The cells of the membrana granulosa were much developed, and those of the discus proligerus extended into fibres, as I have always observed to be the case in those ova which are designed for the immediate act of fecundation. The yolk of one ovum had a spotted appearance, which was not the case with the others. A germinal vesicle was no longer discoverable in any of these four ova, in spite of the greatest care and attention. The ovum from the largest Graafian vesicle measured in the diameter of the discus 0,0100 (Paris inch); of the zona, 0,0060; of the yolk, 0,0045. The zona itself was 0,0006 thick.

I killed this animal at half-past 6 P.M., ten hours therefore after coitus; at which period, in other instances, the ova have always left the ovary. The uterus of the other side had also been removed, and the fallopian tube and the ovary had become adherent. But notwithstanding this, in the latter were several Graafian vesicles, much swollen, which, however, still contained ova; and in three of them the cells of the discus exhibited their spindle-like metamorphosis even yet. The yolk of each of these was spotted very dark; which appearance, however, was not produced by any formation of cells. It rather appeared to me as though a retrograde metamorphosis were already going on in the yolk, caused probably by the general condition of the ovary. Some other Graafian vesicles, for instance, which were not much swollen, contained some of those ova, with a darkly spotted yolk, in which both the cells of the membrana granulosa, and of the discus, were changed into dark granules. In the ova of this side, also, I could no longer find germinal vesicles. Neither of the ovaries exhibited any corpora lutea.

On the 22d April, 1841, I cut out both uteri, leaving the ovaries and tubes, from a rabbit which was eight days gone with young. The animal

soon recovered perfectly, and I therefore, on the 22d of June, placed it again with the male. The sexual desire of the doe was so strong, that it not only permitted coitus immediately, but during the intervals imitated the act upon the other does. Four days afterwards I killed it. I found on the left ovary three, and on the right five, recent corpora lutea, and in addition a Graafian vesicle filled with black coagulated blood. Satisfied with this result, I unfortunately only examined the tube with reference to ciliary motion, which it exhibited very strong and perfect.

In the summer of 1841 I had excised the uterus of the right side from another rabbit, leaving the fallopian tube and ovary. This animal subsequently exhibited strong signs of being "at heat," drove the other does about, mounted on their backs, and so on. Notwithstanding this, so often as I placed it with the buck, it would not permit the act of copulation to be effected: such was the case on the 15th and 16th of May, 1842. I had quite forgotten the former operation upon it. I allowed it to remain with the male unnoticed from the 17th to the 21st of May, and on the latter day made use of it for another experiment.

I now found to my surprise the traces of the operation; and, in addition, that coitus had notwithstanding taken place.

In the upper portion of the left uterus I found an ovum, which attracted notice from its slightly swollen and transparent condition. The stage of its development corresponded to that usually observed in the ova of rabbits about the 9th day. The ovary exhibited a corpus luteum. The right uterus was wanting, and the lower end of the tube was adherent to the lower portion of the excised uterus, and both were closed. Between them was a thick cheesy mass, which, when examined under the microscope, exhibited pus globules, and within which the ligatures made use of in the operation were inclosed. The ovary presented four corpora lutea, just as perfectly developed as that of the left side. It occurred to me to examine the fallopian tube, and I unexpectedly found the four ova in the centre of it. They had manifestly advanced a certain extent in their development, had then remained stationary, and were now on the point of aborting. The discus proligerus had disappeared, and in its place a slight layer of albumen had formed around the zona; the latter was somewhat swollen. The vitellus did not completely fill the zona, and clearly showed signs of resorption. It was irregular, very pale, granular, and small. Some darker granules were remarkable in the yolk of one of the ova.

On the 19th and 20th of January, 1843, I removed, in four portions, both the uteri (with the exception, as will afterwards appear, of a small portion at either end of each) from a Pomeranian bitch, which had copulated three weeks previously; they contained five ova. The animal bore the operation very well; and as it was my intention to let it live, I

brought all the ligatures out at the external wound, and closed it by suture. It was soon quite well, the ligatures came away, and the external wound closed. I then took the animal to live in the house, where it became lively to a remarkable degree, and grew large and fat. A certain sensibility of the belly on pressure, which had existed, also disappeared, and in a short time not the slightest trace of its having suffered anything could be remarked.

On Sunday, the 14th of May in the same year, I first observed the dogs begin to follow her. She bit them off, however, in a playful manner, and continued very lively until Wednesday, when the external genital organs were much swollen, and secreted blood in a considerable quantity. On this day the animal became dull, and ate nothing; but on the next it became again very cheerful, and the dogs were still more zealous in following her about. She would not, however, permit the act of coition, until the evening of the 20th, when it took place in my presence, and continued during three-quarters of an hour. From that period she permitted it daily with different dogs, through the whole of the following week, until the 28th.

On the 29th also she was willing, but the male did not accomplish the act. I was very undecided as to the time at which I should examine the animal. I was convinced that the ova would have separated from the ovaries, and have entered the fallopian tubes. I further expected that they would abort there; but I much wished to wait until the period when, in the regular course of things, a division of the vitellus should have commenced, in order that I might see whether perhaps some trace of such division might not make its appearance, as is often the case in the unfecundated ova of frogs and fishes. My reason for allowing the animal to live so long was, because this division first commences in the lower portion of the tube, and bitches generally permit copulation until the period when the ova are on the point of entering the uterus. As, however, sexual impulse had continued in this case already eight days, I was in fear for the ova, and had the animal killed at ten o'clock on the 29th.

On opening the abdomen, the traces of the previous excision of the uterus were on the whole but slightly marked. The bladder adhered anteriorly to the cicatrix in the abdominal parietes by a sort of mesentery: all the other organs were free from such adhesions. The great omentum was completely united with the remains of the uterus and its mesentery, as also with the investing membrane of the left ovary, and altogether enveloped in a peculiarly complete manner the entire alimentary canal; the latter, however, was throughout quite freely moveable. With regard to the organs of generation, the vagina, the os uteri, and body of the uterus, were perfectly normal in condition. There remained attached to the body of the organ, on the right side, about an inch, and on the left half an inch, of the cornu, which parts were also perfectly normal,

and both completely closed superiorly. On the right side a callus-like portion of cicatrix material (*Narbensubstanz*), two lines in length, connected the lower portion of that cornu of the uterus in direct continuation with the upper portion, which was more than an inch in length, and had the ovary and fallopian tube attached to it, both of which were quite normal, and had acquired no adhesions. The left side presented a larger portion of this cicatrix-material, and between that substance and the omentum the most complete adhesion had formed: with this material, the upper portion of the left cornu, with ovary and tube attached, was continuous, as on the other side; the upper portion of this cornu, however, was somewhat distended, and contained pus. The ovary and tube were quite normal.

My first investigation was with respect to the seminal fluid; and I found spermatozoa in the body and both the lower portions of the two horns of the uterus; on the whole, however, but few. They were no longer in motion; which fact, as well as their paucity, astonished me in some degree, as the bitch had copulated only the previous day. There was not the slightest trace of spermatozoa to be found in the upper portion of the right uterus, after the most minute search; the pus on the left side made the examination both impossible and unnecessary.

The ovary of the right side exhibited two, and that of the left four, quite perfectly developed, fresh and large corpora lutea. They did not in any point differ from the corpora lutea of other bitches on the eighth day after coitus: they were quite as large, quite as much injected and flesh-coloured in appearance; they presented in their interior precisely the same structure and radiated texture, and contained also a small central cavity, filled with a transparent gelatinous mass. The surfaces of most were smooth, only one of them exhibiting a kind of opening; such an opening is, however, rarely met with in bitches on the eighth day, and is in most instances already closed, without even a trace of its appearance. Lastly, both ovaries still contained most distinctly the five corpora lutea of the impregnation in January, in the form of lenticular yellow masses.

I next examined, in the most careful manner possible, both fallopian tubes, and the upper portion of the right uterus, for the ova. Unfortunately, however, I found none. I do not think that I could have overlooked them, even although they might have already undergone an essential change in appearance. On the other hand, I am nevertheless convinced, from the condition of the corpora lutea, and from my experience with rabbits, that the ova had already left the ovary, and had entered the tube, perhaps even the uterus, and had there aborted for want of fecundation. I ought to have made the investigation earlier, on the third or fourth day, and not to have deferred it until the ninth.

On the 14th August, 1842, I opened the abdomen of a bitch, supposing

her to be with young, but found such was not the case. I then put a strong ligature around the left uterus, at about its middle, cut off the ends of the ligature, and the animal recovered.

The same bitch copulated on Monday, the 4th of January, 1843, at nine o'clock A.M., and again on the day following twice, with different dogs.

Friday, the 13th January, I had her killed. In the middle of the left uterus, at the part where the ligature was applied, there was a swelling of the size of a walnut, to which the omentum was firmly adherent: a small portion of the organ above the ligature retained its normal diameter, but the upper end of it appeared very much stretched, and expanded into a fluctuating sac, almost as large as a hen's egg, into which the fallopian tube conducted. The right ovary presented three corpora lutea, the left four, quite as perfectly formed as the others. I first examined the right uterus, and found the three ova, one about the middle, the second about an inch from it, and the third above, in the point of the uterus. They presented that state of development at which the germinal membrane is just commencing to form itself from the vitelline globules, and which I will describe more precisely in another place. On the left side I unfortunately examined the fallopian tube in the most careful manner in vain—I could find no ova. Meanwhile, since they had certainly been extruded, as the four corpora lutea proved, I think that they had passed through the fallopian tube, and had reached the sac-like expansion of the uterus. This, however, was filled with a stinking green pus (the swelling at the point of ligature also contained some), which necessarily rendered all search after the ova impossible.

I think these experiments of my predecessors, together with my own, prove most decisively, that, even though the advance of the seminal fluid of the male through the fallopian tube up to the ovary, and its influence upon the ova, be prevented, yet, on the appearance of “the heat,” and in the foregoing instances after reception of the male, all phenomena develop themselves in the ovaries and ova notwithstanding, just as in the perfectly normal condition. The ova mature, the Graafian vesicles swell and open, the corpora lutea form, the ova escape and reach the tube, and some of the phenomena of development even commence: as, however, the influence of the semen on the ova is prevented, that process does not advance farther, but the ova retrograde, dissolve, and abort. This last circumstance proves the whole succession of these phenomena to be altogether independent of coition, and to be solely and abstractedly founded on the progress of development of the ova. Since I have proved the material contact of the seminal fluid with the ova, all thoughts of an “aura seminalis,” of a resorption of the semen, or a still more mysterious influence of copulation, which previous observers declared to be demonstrated through these very same phenomena, are altogether out of the question, and especially

refuted in these particular instances, by the fact, that the ova, in consequence of the impediment to that material contact, were not endued with the capacity of development, and aborted. Had the extrusion of the ova, the development of the corpora lutea, &c. in these instances been conditionate on the semen and coition, the ova would have become fecundated also, and have advanced in their development. I consider, therefore, that notwithstanding the intercurrence of copulation, these experiments prove the independent progress of development of the ova.

Another observation which I was so fortunate as to make, shows, however, still more decidedly, how independent the maturation and extrusion of the ova are of coition.

With a view to ascertain the point to which the seminal fluid penetrated in the bitch immediately in the act of coition, I obtained a strong and vigorous young one, which had never borne young. In order that I might know with perfect certainty when the first act of copulation took place, I kept the animal about me in my house, and watched it closely. At the beginning of June, 1843, I remarked that it was about to be "in heat," for the dogs began to follow her about, and a flow of blood took place from the vagina. On Friday, June 9th, however, the animal would on no account permit coitus to take place. It was therefore chained up, and kept strictly isolated until mid-day on Sunday. At three-quarters past 1 o'clock on that day I again brought a dog to her, and she then for the first time received the male.

Immediately afterwards I cut out the left uterus, ovary, and fallopian tube, closing the wound by suture. I first examined the uterus, and found it quite filled, even up to its extreme point, with lively-moving spermatozoa. My next object was to examine the tube, in order to see whether the semen had already penetrated into it also: as I was preparing this, and laying bare the ovary, I saw to my surprise that the ova, which I had confidently expected to be yet in the Graafian vesicles, must have already left the ovary. I observed upon the ovary five small red openings, from one of which a small red mass grew forth, and saw that five Graafian vesicles had already opened there. The formation of the corpora lutea proceeding from the bottom and walls of the follicle was already far advanced; at the same time, they still contained a considerable cavity filled with limpid serum, which however no longer held an ovum. This afforded me additional evidence of the probability that such a condition of parts had led previous observers, who were unacquainted with the ovum itself, to think that the follicles were not as yet opened. I forthwith, however, became fully convinced that the opening had taken place, for I found the five ova close together, already advanced nearly two inches in the tube. The examination of them yielded nothing novel. Their condition was altogether that which I have always seen in the ova at the same stage, and

as usual perfectly resembled the fully matured ovarian ova. On the other hand, I sought through the entire length of the tube, down to the ostium uterinum, for spermatozoa, without success ; no such a thing was to be seen anywhere, and I devoted so much time and care to it, that I venture to assert positively that the seminal fluid had not yet penetrated into the fallopian tube.

On the following morning, at 10 o'clock, after 20 hours therefore, within which time, according to my previous observations, I had found that the semen can reach the ovary, I had the animal killed. The right ovary also exhibited five small openings, and five corpora lutea, which were more advanced in their development than those of the opposite ovary, and in addition to these one very large unopened Graafian vesicle. This fallopian tube also contained five ova, which had advanced more than half way down it, and were several lines distant from each other. Three of them were perfectly normal in condition, and very like those of yesterday. Two, however, were distinctly abnormal and abortive, the zona being indistinct ; the discus proligerus very incompletely developed ; the yolk a small irregular mass of vitelline granules. I now also found spermatozoa in the tube, some of which were still in motion, but they had only penetrated about three lines from the ostium uterinum. The tube did not contain any others throughout the rest of its extent, neither was there any trace of them around or upon the ova, so that the latter were certainly not yet fecundated.

I consider the foregoing investigation affords irrefutable proof that the ova, after having reached maturity, quit the ovary, and enter the fallopian tube, independently of all influence of copulation. That that act was not accomplished earlier than it was observed, may, from the measures adopted, be assumed as certain. That the ova might have been extruded during and in consequence of coition, cannot at all be assumed, for the following reasons. In the first place, it is certain that that result does not always attend the act, as I have myself found the Graafian vesicle still closed in other bitches after they had received the male several times ; and in the second place, it is not conceivable that the ova should have travelled a distance of two inches through the narrow fallopian tube, in the short space of a quarter of an hour, whilst they require about eight days to traverse the remaining two to three inches of its length.

If, then, in this instance, the ova had actually left the ovary quite independently of copulation ; if they must be considered to have reached the fallopian tube unfecundated, and to have remained so even after 20 hours ; the next question is, how to reconcile this with my previous observations, in which I found the Graafian vesicles to be still closed in bitches 6, 18, 20 hours after the first coitus, and the semen to have penetrated through the entire tube, even to the ovary.

The solution of the question is manifestly this : that the ova, when once matured, have a certain scope afforded both as to time and place for their fecundation. It depends, as it would seem, upon the individuality of the animal, and upon the opportunity, as to whether coitus be permitted and accomplished when the ova are still in the ovary, or when they are already extruded and have reached the fallopian tube. If the natural habits of the animal be not interfered with, and the opportunity for copulation be free, it appears that sexual impulse manifests itself before the ova are extruded. If coitus take place at that period, the semen has sufficient time to penetrate through the fallopian tube to the ovary, and this can take place in the dog in 20 hours, as my previous observations show. Other individual animals do not perhaps permit the act until a later period, or the opportunity fails, as in my dog, because they are confined. The ova in such case are extruded notwithstanding ; they may also still become fecundated on copulation taking place. I cannot determine with precision the range of time within which this is possible. As, however, bitches usually receive the male during eight days, and as the first more precise phenomenon of development of the ova, viz. the division of the yolk, commences in the lowest part of the fallopian tube, where they are to be found about the seventh and eighth day, this would seem to denote the limits of the capacity for fecundation (*befruchtungs-fähigkeit*) of ova in the dog.

Since I gained an insight into these circumstances, my attention has been drawn to many results of my former experiments, which I had previously valued but slightly, and put a different interpretation upon. I was, as I have said before, prepossessed with the opinion that coition was the cause of the extrusion of the ova from the ovary. I therefore, like all my predecessors, always calculated from the first act of copulation. It happened accidentally that most of the animals which I made use of, especially for this subject, had probably received the male before the ova were extruded. But I now find amongst my observations several instances in which I have remarked that, although the ova were found in the upper third of the tube, yet I saw spermatozoa only in its lower part. When once, however, I became acquainted with the fact that the latter might advance through the entire length of the tube, I thought I must have overlooked them in the upper portion in the above instances—perhaps because they were few in number, or I had not been sufficiently careful, and so on. But I am now convinced that such were instances in which the ova had passed out before coitus had taken place, and the seminal fluid had time to advance higher up into the tube. As a rule, I have also seen spermatozoa upon the ova in the lower third of the tube only ; more rarely in the parts above.

With respect to rabbits, (in which animal we cannot ascertain the

presence of "the heat" so precisely as in the bitch), when the doe receives the male, it seems that the ova are not, usually, extruded before the semen has had time to reach the ovary, for which, according to Barry and myself, nine or ten hours are required. In rabbits I have also always found spermatozoa upon the ova in the upper third of the tube; in them, also, the division of the yolk commences higher up in the tube, and probably, therefore, the period during which the ova are capable of fecundation is much shorter than in the dog.

It follows, then, from all the foregoing, that the mode of calculating the period of the extrusion of the ova, according to the first act of coition, and which has been hitherto universally the custom, is altogether uncertain, and can only, in a degree, approximate the truth.

If, then, according to the above, it be certain that the ova may quit the ovary previous to coition, and enter the fallopian tube, so there can be no doubt but that this also takes place when the act of coition is not effected at all, in which case, however, the ova of course abort. But very few data are to be found in previous authors which bear upon this point, and those naturally do not refer to the ovum, but to the Graafian vesicle and corpora lutea. According to Kuhlemann (*Observat. quæd. circa negot. generationis*, p. 15, *Epicrisis*), in sheep which are "at heat," and which do not receive the male, or at least are not impregnated, no Graafian vesicles burst, and no corpora lutea form; at least he signifies that such only occurs as an exception.

Some experiments of Hausmann's unfortunately have not that demonstrative force which they would have possessed, had not the author slighted and denied the existence of the ovarian ovum. A bitch was "at heat" from the 29th of October until the 4th of November; coitus, however, was not permitted. On being killed at that date, the ovaries presented "Graafian vesicles, which had the appearance of corpora lutea, but exhibited no point of rupture, and contained a limpid fluid but no ovum," (l. c. p. 73, exp. 25.) In another experiment (p. 87, exp. 41) a sow was "at heat" on the 23d of June, but was not admitted to the boar. It was again "at heat" on the 9th of July, and would have received the male, but was not allowed. On the 12th coitus was permitted, and the animal was opened twenty minutes afterwards. The left ovary exhibited five mature Graafian vesicles, which had not burst, and six corpora lutea; the right also presented five mature vesicles, and three corpora lutea. Hausmann is of opinion that these corpora lutea owed their origin to the animal being "at heat" on the 23d of June.

With respect to the sheep, Hausmann says (l. c. p. 94):—"When the sheep does not receive the male, or is not impregnated, no Graafian vesicle bursts; yet it seems that, after frequently returning and unsatiated sexual desire, a Graafian vesicle does burst; after which an imperfectly developed

corpus luteum forms.” And in exp. 56, p. 95, he found close to a corpus luteum, in the left ovary of a sheep which was then “at heat,” and which had also copulated, another such body, less perfectly formed, which he himself considers to be a proof “that a Graafian vesicle may at times open without copulation.” In exp. 58, p. 96, he farther states, that a corpus luteum was found on the ovary, near to a Graafian vesicle which had burst as the result of former coitus, “although the sheep had not received the male, but had several times been ‘at heat.’”

The following are my own observations in reference to this part of my subject :—

At four o'clock in the afternoon of the 7th December, 1843, I obtained a lamb, which had presented signs of being “at heat” about an hour previous, but which had not received the male. The animal was forthwith shut up alone. The following morning I admitted the male to it; he several times shewed a disposition to effect the act of coition, but was prevented. Since, according to Kuhlmann (l. c. p. 13, note), sheep remain “at heat” during twenty-four hours only; since Kuhlmann and Hausmann profess to have seen the Graafian vesicle open twelve hours after coitus, and Von Baer has observed the ovum of the sheep in the fallopian tube before the end of the first day afterwards, I thence concluded that the ovum would quit the ovary during the first twenty-four hours of the period of “the heat,” and therefore had the animal killed between three and four o'clock in the afternoon.

The same afternoon I observed, to my great satisfaction, that a Graafian vesicle on the right ovary had burst. The spot did not project upon the surface of the ovary, but attracted observation by a delicate lively red circle of vessels around a small opening, an appearance already familiar enough to me in dogs and rabbits; but one which may be easily, and certainly has been very often, overlooked by such as are not experienced in similar investigations. The diameter of this small opening was about $\frac{2}{5}$ P. line, = $\frac{1}{10}$ millim. I then searched throughout the vagina and the entire uterus most closely for spermatozoa, in order to gain the additional negative certainty that no act of coition had taken place; naturally enough, however, I could not find a trace of them, since it had been rendered absolutely impossible.

On the following morning I searched for the ovum. I laid the fallopian tube upon a plate of glass, spread out the fimbriæ of its infundibulum carefully, and examined them first with the simple microscope. I found nothing there, but remarked a thread of mucus (*schleimfaden*) gradually extending itself into the infundibulum, the particles of which, when viewed under the microscope, seemed to me to have a great resemblance to thrust-off cells of the membrana granulosa of the Graafian vesicle. I then cut open the first third of the tube with a pair of fine scissors,

separated the margins, and examined every little fold carefully with a magnifying glass, and the assistance of a fine needle. I was so fortunate as to find the ovum in that part of the tube, at a distance of 5 P. lines, $= 11\frac{1}{4}$ millim., from its entrance. I consider this as an extremely lucky occurrence, notwithstanding my acquaintance with the object; for the investigation is really so difficult, that one cannot wonder when nine out of ten times the search proves fruitless.

The ovum, when removed from the tube, still presented, under the microscope, altogether the appearance of an ovarian ovum. The zona was still surrounded by the cells of the discus, and although they were not extended out into fibres, yet they had perceptibly commenced to coalesce. The diameter of the ovum with the discus was 0,0079 P. in., $= \frac{1}{11}$ P. line, $= \frac{11}{50}$ millim. The yolk completely filled the interior of the zona, in the form of a finely granulated, slightly obscure mass. After I had separated the cells of the discus from the zona with a fine needle, I found the diameter of the ovum in the zona to be 0,0054 P. in., $= \frac{1}{15}$ P. line, $= \frac{7}{50}$ millim. The thickness of the zona itself was 0,0006 P. in., $= \frac{1}{133}$ P. line, $= \frac{1}{59}$ millim. I could not perceive a germinal vesicle shining through the yolk, though it is generally possible in the ovarian ovum of the sheep.

After the ovum had remained for a time in contact with an aqueous solution of albumen, with which some common salt was mixed, the following very remarkable changes were developed in it. Firstly, an endosmosis took place into the zona, whereby it was distended, and the ovum assumed a somewhat elliptical form, measuring after a time, in its larger diameter, 0,0071 P. in., $= \frac{1}{12}$ P. line, $= \frac{19}{100}$ millim., and in its smaller, 0,0060 P. in., $= \frac{1}{13}$ P. line, $= \frac{8}{50}$ millim. As a farther result of the same action, the yolk no longer filled the interior of the zona completely, but shrank away considerably from its inner surface. In the interspace between it and the zona, a small somewhat yellowish shining vesicle or granule, having a pretty strong refracting power, now became visible; it measured 0,0005 P. in., $= \frac{1}{167}$ P. line, $= \frac{1}{77}$ millim., and corresponded accurately to what I have seen and described as existing in the neighbourhood of the yolk when it no longer fills out the zona, in the ova of rabbits and bitches found in the upper third of the fallopian tube. I have started the supposition that this corpuscle might be the nucleus of the germinal vesicle, which had itself undergone liquefaction, and confess that I find this opinion rather strengthened by the investigation in question; for in this instance it was extremely probable that the germinal vesicle had disappeared but a very short time.

In addition to the above phenomena, the yolk of this ovum, after its separation from the zona, presented the most deceptive appearance of an especial vitelline membrane that has ever presented itself to my notice. I remember, also, that several observers, Professor Bruns, for instance, state

that they have seen this vitelline membrane particularly distinct, and especially in the ovum of the sheep. The yolk had a very sharp outline, which, under a certain adjustment of the microscope, appeared like a dark line, as though formed by a membrane. It seemed even as if an interspace existed between it and the granular vitelline mass. Notwithstanding, however, I remained convinced, even in this instance, that the yolk possesses no especial vitelline membrane besides the zona, but rather that it is a gelatinous or albuminous sphere, in which the vitelline granules are suspended and distributed. The fewer the latter in proportion to their connecting material, the less opaque and dark is the yolk; as is the case, for instance, in the ovum of the sheep. The surface and margin of a section of the sphere are for the most part formed of this connecting material, and give it therefore a very sharp outline, which appears to be produced by a fine transparent investment. That this is the correct explanation is proved, in the first place, by very careful observation with a good microscope; by the aid of which, and by very gently changing the focus, one can directly convince oneself of the relation of parts just now described. In the second place, by continued observation, I saw clearly how the yolk gradually absorbed the fluid which had penetrated into the zona. Here and there it swelled up to a certain extent; the sharp outline was lost, whilst it remained at other points; so that this could not possibly have been caused by the bursting of a delicate investment. Thirdly, after I had burst the ovum by means of the compressorium, some of the separate fragments of the yolk even exhibited the same sharp margin, which could not therefore be produced by an investing membrane. Lastly, the before mentioned granule, near to the yolk, proves that the latter could not possess an especial covering; for the granule must, at all events, have been previously within the yolk, although enclosed immediately upon its surface, but it was now free in the interspace between the yolk and the zona. Had the yolk possessed an investing membrane, this would have been impossible.

I attach great importance to this point, since the question respecting an especial vitelline membrane is decisive in reference to the further development of the ovum. As I must in the prosecution of this examination from time to time contradict the existence of such a structure, I wished that others might also be strengthened or corrected in their views.

The above described observation, made upon a young animal, which had never been impregnated, and never (before) been "at heat," proves altogether indubitably, and in the most complete manner, the maturation and extrusion of the ovum from the ovary, quite independently of coition.

On the 18th and 19th December, 1843, I first remarked that a large bitch which I had in my possession commenced to be "in heat." The vulva was much swollen, and the dogs followed her eagerly. On the

19th, I tried if she would receive the male. But, although she played about with him actively, she would not permit coitus to take place. I then kept her closely shut up, and on the 21st admitted a dog to her again. She now seemed by her actions to be willing to receive the male : I did not, however, permit it, but separated the animals again. At ten o'clock in the morning of the 23d, I cut out the left ovary and fallopian tube, and closed the wound by suture. It proved on examination that the Graafian follicles had not yet opened ; four of them, however, were much swollen, and had a diameter of 2 to $2\frac{1}{2}$ Par. lines = 4 to 5 millim. I removed them cautiously from out of the stroma of the ovary, and placed them, as cleanly dissected as was possible, upon a small plate of glass. As I opened the first, an ovulum, provided with its discus, escaped with the fluid ; it had a diameter of 0,00078 P. inch = $\frac{1}{50}$ millim. To my astonishment, this Graafian vesicle contained a second ovum, with a discus of 0.0081 P. in. = $\frac{1}{4}$ millim. in diameter. The three other vesicles also each contained an ovum of about the same size. The inner surface of the Graafian vesicle was already lined with delicate granulations, the elements of the mass forming the corpus luteum, which, as it appears to me, develop themselves from the cells of the membrana granulosa ; I observed here, very distinctly, in several Graafian vesicles, the manner in which the ovula are stowed in them. The cells of the discus form a small cone, the roundish head of which receives the ovulum, with which it projects free into the cavity of the Graafian follicle, the latter being filled with fluid ; with its basis it rests upon a point of the wall of the follicle, probably at the precise spot where the latter opens.

These ova, moreover, appeared evidently not yet fully matured for their exit. The Graafian vesicles, for instance, were not only not much thinned, but the cells of the discus were also not extended out into those fibres which denote the full maturity of the ovum. When they were removed with the aid of a needle from the zona, the ova in the latter measured 0,0060—0,0065 P. In. = $\frac{8}{50}$ — $\frac{1}{8}$ millim. The yolk completely filled the interior of the zona in all of them, and only in one little spot had the vitelline granules receded from it, as though indicative of the situation of the germinal vesicle. In these instances I could neither obtain an accurate sight of it so long as the ova were yet closed, nor when I opened them with the compressorium. I think I may feel convinced from the foregoing details, that had the bitch received the male then, the spermatozoa would certainly have had time to reach the ovary before the Graafian vesicles had opened.

I did not kill the animal until five days afterwards, in order to be certain that the opening of the follicle had taken place. The very first sight of the ovary, indeed, showed that it had opened. The tunica vaginalis of the ovary, formed by the peritoneum, contained a considerable

quantity of limpid serum. Four corpora lutea were well developed. In two of these, the mass of granulations forming them grew out a considerable distance from the previous opening in the follicle, in a manner which I have seldom seen in the dog. The opening had probably closed at an earlier period in the other two, before the mass of the corpus luteum had arrived at so considerable a growth, and they were therefore now imbedded in the stroma of the ovary.

After I had carefully dissected the fallopian tube, I extended it out upon a wax tablet, and opened it with a pair of fine scissors: I found the four extruded ova already far advanced in its cavity; they were close together at a distance of 3 P. in. = 8 centim. from the ostium abdominale. The tube itself was 5 P. in. = 13 centim. in length. Three of these ova had the usual normal round appearance; the fourth, on the contrary, had an anomalous form, such as I had often previously seen in the dog, being elliptical, or almost like the figure of a guitar. All still had their discs around the zona, yet it was clear that these cells no longer retained their full, normal appearance, but had already commenced to undergo liquefaction. The ova had somewhat increased in size, for they measured with the discus 0,0090 — 0,0097 P. in. in diameter. Such was not the case with the yolk, for that seemed rather to be somewhat condensed, and on that account it no longer completely filled out the zona in all the ova. It had its usual form, and no trace of a division was developed in it. In the interspace between the yolk and the zona in one ovum, that large vesicle or granule ($\frac{1}{140}$ P. in. = $\frac{1}{62}$ millim.) which, as it seems, is always observable in ova at the commencement of the tube, presented itself: I have on a former occasion pronounced it to be the nucleus of the germinal vesicle. I saw nothing of the sort in the other three ova. I no longer found any trace of the germinal vesicle itself in any of the ova.

I do not believe it to be possible to prove the entire process of the maturation and exit of the ova during "the heat," and without coition, more completely than it is done by this twofold observation instituted in one and the same animal.

On January 4, 1844, I obtained the genital organs of a sow, which had manifested the strongest signs of "the heat" during 48 hours, but had not been admitted to any male. The Graafian vesicles, however, were still closed. A considerable number of these, indeed, were much developed in both ovaries, and were very conspicuous from the rest, in consequence of their greater vascularity; but none of them had burst, and I forthwith obtained the ovum from one which I had removed from the ovary and opened upon a glass. It was, as usual, surrounded by the cells of its discus, which, moreover, were still round, and not extended into fibres. It measured in the diameter of its zona 0,0060 P. in. The yolk, which consisted for the most part of rather large fat globules, did not completely

fill the interior of the zona. After I had removed the cells of the discus with a needle, and the ovum lay level upon the glass, its diameter had increased to 0,0068 P. in.; the yolk now filled the interior of the zona completely, and as its particles had receded somewhat from each other, a clear circular spot became apparent at one point of its circumference, and which was readily recognisable as denoting the presence of the germinal vesicle, although being covered by the vitelline granules, the outline of the latter was not perceptible. In fact, on crushing the ovum by gentle pressure, the vesicle with its germinal spot came out free; the spot was unusually large, but did not admit of any insight into its structure even under a very high magnifying power.

Thus this animal was evidently killed too early, at a time when the phenomena of "the heat" had not yet advanced to the opening of the Graafian vesicles, and exit of the ova.

Some weeks previous, on December 4, 1843, I had examined the genital organs of another sow, which had been kept distinct from the male from its birth. The possessor of the animal assured me that on former occasions he had several times observed it to exhibit signs of "the heat," and that they had been present again at the time of its death, but he could not afford me the necessary information as to how long they had existed before death. On examining the ovaries, it appeared that fresh corpora lutea were present in both. There were eight on the right ovary. In size they were all equal to large peas, and projected considerably forwards upon the surface of the ovary. They had a dark brownish-red colour. A small bright red coloured spot was observable upon the highest point of each of them, but a distinct opening was no longer perceptible. When, however, the tunica propria of the ovary, with its serous investment, was dissected from off the surface of one of these corpora lutea, it presented a small opening precisely corresponding to the above mentioned spot. The corpora lutea consisted of a peripheral stratum (about a Parisian line in thickness) of flesh-like granulations, just such as are always developed from the inner surface of a Graafian vesicle in its transition into a corpus luteum. This inclosed a considerable cavity filled with a dark red coagulum, which was intimately connected with the granulations. I could not discover an ovum in any one of them. On the left side were two corpora lutea presenting the same condition; near to these, two others, which were considerably larger, and had an almost transparent reddish glistening appearance. The peripheral stratum in these was much less developed than in the others. It enclosed a pellucid, reddish coagulum, and also a considerable quantity of an uncoagulated fluid, similar in appearance, which however coagulated so soon as it was exposed to the air upon a plate of glass. This coagulum was also connected to the walls. These two last described might have been mistaken for Graafian vesicles

which had not as yet burst, for the mass of the corpus luteum commences its development from the walls even previous to the opening of the vesicle; such is the case at least in the bitch. But, in the first place, the small red spot on their summits, the point of opening; secondly, the extravasation of blood, which had taken place into their interior; and thirdly, the absence of an ovulum (which, at least, I could not find) may be received as evidence that both these vesicles had also already burst; but that then (as I have also seen in bitches in which I found the ova in the fallopian tube) the opening had again closed, and a second secretion of liquor sanguinis had taken place subsequently into the evacuated vesicle, whereby it had once more become distended.

Unfortunately I did not succeed in finding the ova in the tubes, although I searched for them carefully for several hours. But it is a task of so much difficulty, both from the considerable width and length of the tube, and the many folds which it has, that I do not consider this negative result as proving anything. Besides, it is probable, that the ova, being unfecundated, had already undergone liquefaction. The advanced development of most of the corpora lutea, as well as the fact of the uterus and vagina no longer presenting any striking degree of turgescence, seemed to indicate that "the heat" had already some time since passed over.

I saw completely developed corpora lutea in the ovaries of another sow, which had likewise never received the male, and which had been observed to be "at heat" fourteen days previously, but I felt that it would be to no purpose to search for the ova.

Soon after, however, I obtained the genital organs of a young sow which had never been with young, and which I knew for certain had been shut up alone for the previous thirteen days. The first traces of "the heat" appeared five days before, and after they had commenced to pass off, the animal was killed (on the morning of the fifth day.) It was evident, at the first glance of the ovary, that the ova were already extruded, for seven recent corpora lutea were developed on one of them, and on the other, six. There was no longer any opening perceptible in them, nor did they any longer contain large cavities filled with blood or serous fluid, but the Graafian vesicles were already filled up by granulations of the usual character. The point of rupture, however, was still distinctly recognisable from its deep redness. I proceeded at once to make an accurate investigation of the fallopian tube, which was about 11 Paris inches in length, and very full of folds, particularly in the first half of its length. By cautiously removing the epithelium in small portions, and examining them with a single microscope, I succeeded in finding ten ova in the lower portion of the tube, at about from two to four inches from the ostium uterinum, and at some distance from each other. When examined with the microscope, they bore a general resemblance to an ovarian ovum when

deprived of its discus proligerus. They had a diameter of from 0,0064 to 0,0068 Paris in. (about equal to $\frac{1}{13}$ Paris line, or $\frac{1}{6}$ millim.) There was no trace of albumen developed around the zona, but the latter formed the sole investment of the ovum, and had in most instances a thickness of 0,0005 Paris inch = $\frac{1}{17}$ Paris line, = $\frac{1}{8}$ millim. The yolk did not completely fill the interior of the zona in most of the ova, and varied in its diameter from 0,0040 to 0,0054 Paris in. = $\frac{1}{20}$ to $\frac{1}{13}$ Paris line, or $\frac{1}{8}$ to $\frac{7}{50}$ millim. Its composition exhibited those larger fat vesicles which also distinguish the ovarian ovum of the sow; they were, however, in most of the ova, so unequally distributed through the vitelline mass, as to give it an irregularly spotted appearance. The outline of the yolk was in this instance again so sharp, particularly in a certain adjustment of the microscope, that the existence of an especial vitelline membrane might have been assumed (as has, indeed, been the case with Dr. Meyer in reference to the ovum of the sow); but the same circumstances which I have already mentioned above, in reference to the ovum of the sheep, here again afforded complete proof that such was really not present. I could no longer discover anything of the germinal vesicle; here, again, however, there was in some of the ova a very pale granule between the yolk and the zona, which, as I have before stated, might correspond to the nucleus of the germinal vesicle, the germinal spot.

These three observations, then, upon the sow, furnish, so far as regards that animal, the most complete proof of the law laid down by me.

I think that the following observations upon a rat may also be classed with the foregoing. The animal was taken in a trap during the night between the 1st and 2d of February, 1844, and remained in it alive until 11 A.M. on the morning of the 3d. On examination after death, which was effected by drowning, it appeared that numerous large and recent corpora lutea were developed in the ovaries. Inferring from that circumstance that the animal had been impregnated only a short time previously, I examined the vagina, uterus, and fallopian tubes, most minutely, but could not find any trace of seminal fluid in either of them. I discovered, however, the extruded ova in the commencement of the tube. I may observe, that the ova are extremely difficult to find in the oviduct, both in this animal and the mouse. The tube is so small that it is not possible to cut it open with even the finest scissors. The observer therefore has no other resource left him but to dissect out, in the first place, all its tortuosities carefully, which of itself is no easy task, they being very closely compacted, and the tube measuring scarcely half a Paris line in diameter, and then to remove the contents of the oviduct, by very cautiously and gently scraping small portions at a time, on to a plate of glass, examining them afterwards most minutely with a simple microscope. It is very difficult to detect the ova even then, for they are not only very

small, but almost perfectly transparent also, the yolk being composed of only a very pale minutely granular substance. I was, however, as I have stated, fortunate enough to find the ova in the superior third of the tube. They measured, including the zona, 0.0041 P. in. $= \frac{1}{250}$ P. line, $= \frac{1}{9}$ millim. in diameter. The zona was about 0.0002 P. in. $= \frac{1}{4000}$ P. line, $= \frac{1}{168}$ mil. in thickness. There was no longer any trace of the cells of the discus around the zona. The vitelline mass completely filled out the interior of the zona in all the ova; and I could not discover any thing of a vesicle, corpuscle, or the like, in them. I should attach great importance to the latter circumstance, if the observation itself did not admit of a doubt with regard to its correctness; for the fact of the yolk being so very transparent ought to render the observation of those changes which occur in the germinal vesicle or germinal spot, previous to the commencement of the division of the yolk, much more easy in this than in any other mammalian ovum as yet investigated. The contents of the oviduct are, however, so small, that the ova cannot be examined at all without the addition of some fluid; and it becomes a question as to what changes that (in this instance it was saliva) might have already induced in the interior of the delicate ovulum. An ovarian ovum which I examined had about the same diameter; the yolk, was, however, still more pale, and I most distinctly recognized in it the germinal vesicle, measuring 0.0011 P. in. $= \frac{1}{70}$ P. line, $= \frac{1}{31}$ mil. with its germinal spot of about 0.00039 P. in. $= \frac{1}{2500}$ P. line, $= \frac{1}{98}$ mil. The germinal spot refracted the light strongly, had a very dark outline, and resembled a fat globule.

I think that this rat had not been impregnated, but that the ova had quitted the ovary and entered the fallopian tube notwithstanding. Had copulation been effected, according to all that I have observed on this point in other animals, the spermatozoa would have been discoverable somewhere in the female organs of generation, for the ova were yet in the superior part of the oviduct. In a mouse, in which, about two years previously, I had found the ova near the same spot in the oviduct, the entire uterus and the tubes were distended with seminal fluid, and spermatozoa were also present upon the zones of the ova. Probably, therefore, this rat was made a prisoner in the commencement of "the heat," before copulating, and the ova had quitted the Graafian vesicles without impregnation.

Previous observers, and among them Barry (Researches in Embryology, 2d series, p. 319, § 161), have frequently observed Graafian vesicles in the ovaries of the rabbit considerably swollen, and filled with blood. Several instances of the same kind have presented themselves to me, but always in does which had remained for a long time separated from the male. I never found an ovum in them, but only blood corpuscles, the figure of which was more or less distinct, definitely preserved, and recognisable. I think it probable that they are Graafian vesicles of a former period of

“heat,” from which the ova have escaped without the act of copulation having taken place. A short time since I examined a rabbit six or eight hours after coitus, and found that the ova had not as yet quitted the Graafian vesicles, which were much swollen. In addition to these, however, there were corpora lutea of a considerable size in both ovaries, which could not be the effect of a previous littering, as they were much too large for that, and the animal had also been kept alone for several weeks previously. I regard these likewise, therefore, as corpora lutea of a “period of heat” which had passed over without copulation.

From all that I have stated, then, it is quite certain, that at the period of “heat,” and even when no act of copulation takes place, the ova of mammalia are extruded from the ovary, and enter the fallopian tube, but that they there prove abortive : corpora lutea, however, are formed upon the ovary as though coitus and impregnation had been effected.

As a counterpart to these observations, which prove the maturation and extrusion of ova independently of the seminal fluid of the male, I will adduce another fact, which proves the same on the other hand for the semen.

On the 6th of March, 1844, I examined a bitch, not knowing when the animal had received the male for the first time, but being certain that coitus had taken place the day before. The left ovary exhibited three corpora lutea, and in the centre of the oviduct I found the three ova quite normal in condition. The right ovary, on the contrary, was very small, and did not exhibit either a swollen or an opened Graafian vesicle, or corpus luteum, and of course there was no ovum in the tube. I found spermatozoa, however, in the uterus and oviduct, not only as high as the ovary, but upon it. This proves positively that no especial design is in operation here. According to the general mode of considering organic phenomena, which always presupposes a precise design, one would have expected that when no ovum was to be fecundated the seminal fluid would not have reached the part. But it had pursued its customary course uninterrupted, just as the ovum takes its own quite independently of the semen. There is no attraction, no polarity, or such like force, in operation here, such as we are so ready to apply to the supposed explanation of certain natural phenomena. Ovum and seminal fluid are altogether independent products of older organisms. Their meeting, and the consequent fecundation of the ovum, although necessary for the maintenance of the species, are in themselves quite accidental occurrences, frequent instances of which are so clearly evident in organic nature.

If, then, the foregoing observations prove, with respect to mammalia, that their generation and propagation are brought about *primo loco* by the spontaneous periodical formation and maturation of ova, and not by

copulation, analogy alone would lead us to presuppose the like for man. But even here indirect proofs at least are not wanting.

It is well known that the function of menstruation in the human female was long ago and frequently compared with "the heat" in animals. Some of the most intelligent physicians and naturalists of various periods have adopted this opinion, although it was contended against by others no less celebrated than themselves. Regarding Burdach as the most important of the latter class, the only argument of all those brought forward by him which seems to me to have any weight, is that in which he draws attention to the important distinction existing between "the heat" and menstruation, in respect to sexual impulse and copulation. That the beast copulates *only* during "the heat;" but man, on the contrary, always feels himself repelled from the female during menstruation. If this were correct, I should also be of the opinion that it involved an essential distinction. But attentive observers have previously remarked, and I can only confirm it, that such a distinction is altogether wanting, for the female of animals also manifests, at the commencement of "the heat," a condition of sickness, during which it altogether refuses to permit coitus to be effected. It seeks that act for the first time only when the phenomena attendant upon "the heat" have reached a certain stage of development. It is, however, also well known that a sensation of unusual good health comes on, and the sexual impulse especially manifests itself in the human female, on the passing off of menstruation; and it seems to me, therefore, that the most complete accordance occurs here, and that all the arguments which have been brought forward in favour of the analogy between "the heat" and menstruation are rather strengthened than otherwise. A repetition of the arguments here would be superfluous.

All intelligent anatomists, physiologists, and physicians, have long since been convinced that the cause of menstruation, as well as that of the sexual impulse, and of the whole female character, is to be sought for in the ovaries. But that the uterus on the other hand,—although it be the organ in which the characteristics of classes and species are most especially manifested, in so far as regards the genital organs,—yet, in the above respect, it has but a secondary importance. Numerous pathological facts of the most diverse description, and primary deviations from the natural formation, exhibit most constant accordance with this view. I shall quote only one such instance here; it is from a recent and almost unknown source of information upon castrated females, communicated by Dr. Roberts, in a work descriptive of a journey from Delhi to Bombay. The persons examined by him were twenty-five years of age, large, muscular, and perfectly healthy. They had no breasts, no nipples, and no hair on the pubes. The vagina was completely closed, and the arch of the pubes

so narrow that the ascending ramus of the ischium, and the descending ramus of the pubes, were almost in contact. There was no deposition of fat about the neighbourhood of the genital organs, nor were the nates more developed than in man, whilst the rest of the body was well provided with fat. There was not a trace of menstrual secretion, nor anything which might supply its place ; there was also no sexual impulse.

Anatomy has also recently furnished direct proofs in favour of the views advanced in this essay. Surprising as it seemed at first, from the endless controversies carried on respecting the corpora lutea, there is now no longer any doubt of the fact that, at each return of the function of menstruation, the ovary takes on a condition of increased activity, that a Graafian vesicle becomes considerably developed, and bursts, and that a corpus luteum is formed in its place. The researches of R. Lee, Paterson, William Jones, Negrier, Gendrin, Raciborsky, and Pouchet, leave no room for doubt on this point. It would occupy too much space to repeat their observations here : I will only mention, that I have myself had the opportunity of making four observations which bear upon this part of our subject, all occurring in young and healthy persons, three of whom met their deaths by drowning, and the fourth died very suddenly. I found indubitable appearances of menstruation in all ; on the ovaries of three of them a Graafian vesicle, which had burst and was filled with blood, and on the fourth an enormously swollen one, which had a diameter of seven lines. I learnt afterwards quite decidedly, with respect to one of these instances, that menstruation was going on at the time when she met her death.

I have just received intelligence from Dr. Ecker, of Heidelberg, that he had found a Graafian vesicle which had burst, and was filled with a recent coagulum, in the body of a person aged 25, who had lately been executed there, and in whom menstruation had been present twelve days before death. He examined the tube with great care in search of an ovum, but in vain ; probably because it had by that time become dissolved. Besides, in my opinion, the human ovum may be classed amongst the most difficult of all tubal ova to discover, in consequence of the slight density of its yolk, and the vaguely defined outlines of its zona.

It is therefore not to be wondered at, that numerous early observers—Valisneri, Santorini, Roederer, Haighton, Home, Brugnoni, Cruikshank, Meckel, Blundell, and others—have seen corpora lutea upon the ovaries of those who had never been pregnant, and even in those of virgins and young girls. Such instances were always regarded as exceptions to the rule—as pathological, or as results of sexual excitement occurring at some former period without coitus—whilst they no doubt were produced by recent menstruation, or the opening of a follicle and escape of an ovum.

Lastly, it is a well-known fact, that women conceive the most readily

immediately after menstruating, and even examples are not wanting where this took place only during menstruation.

It is likewise now a settled point that there is no more certain mode of reckoning the period of pregnancy, than that which dates from the last occurrence of menstruation. I have been informed by Professor Nägele that he had never once been deceived when reckoning nine months and eight days from the last menstrual period in regular instances. At the same time he told me, that he had frequently removed sterility by advising coitus immediately after, or even during menstruation.

Although at the present moment direct information as to the fate of the ovum during and after the menstrual period may be wanting, (for supplying which I hope that an early opportunity, combined with the necessary dexterity and care, may be presented), yet no doubt can be entertained, that—

“In the human female, during the period in which she is capable of childbearing, there occur every four weeks a maturation and extrusion of an ovum from the ovary, which process is accompanied by a simultaneous secretion of blood from the uterus. This periodical maturation of an ovum is the first and most essential condition to conception and pregnancy. At this period only will coitus be followed by conception, at all other times the latter will be impossible.”

I have no doubt that this law will not only stand the test of future experience, but will also serve to explain many phenomena hitherto unintelligible*. Experience will teach us what are the limits of its application in man. It appears that the secretion of blood from the uterus commences whilst the ovum is still inclosed by its Graafian vesicle, and that the latter bursts, and the ovum escapes, towards the termination of that function. While in the tube, the ovum is probably for some days capable of fecundation on copulation being effected. What is the limit to this capacity? Direct observations alone can answer this question, and they are as yet altogether wanting. I believe, however, that I have a right to assume that fecundation must take place while the ovum is in the fallopian tube, since it is extremely probable that development first com-

* Dr. Pank, of Dorpat, lately brought forward a case, in which he considered he had discovered a remarkable organic connection between the fallopian tube and ovary soon after conception, by means of which the passage of the ovum from the ovary into the tube is effected and secured. For my part, I am convinced, and for the following reasons, that this was a case of commencing menstruation, but not one of conception: firstly, coitus was by no means proved to have taken place; secondly, Professor Bidder did not find any spermatozoa in the genital organs; and lastly, because, although the conjectural act of coition should have been effected five days previously, yet the Graafian vesicle was still closed. I consider the connection between the ovary and fallopian tube, which was observed in this instance, as decidedly pathological. Delicate false membranes, of precisely the character described, are very common in connection with the female organs of generation.

mences there. If, from all the information we at present possess, the conclusion may be permitted, that the ovum of the rabbit remains three days, that of the ruminant from four to five, and that of the dog from eight to ten days in the oviduct, we might then assume the ovum of the human female to be capable of fecundation during a period of from eight to twelve days after its extrusion from the ovary ; thus probably, also, from eight to twelve days after the cessation of menstruation. Such a conclusion, however, being based upon analogy, is hazardous, for we know that very great varieties exist in different animals with respect to the time during which the ovum remains in the oviduct ; for example, in the deer, according to my own observations and those of Dr. Ziegler, it most probably requires even months for its passage through the tube. All the attempts to define this period which have hitherto been promulgated,—where ova, 12, 14, and 21 days old, are reported to have been seen in the uterus, and in which even the embryo was already far advanced in its development,—are altogether unworthy of confidence, since they date from the act of coitus, which only specifies the limit of fecundation, but not the period at which the ova quit the ovary, nor do they admit of any conclusion as to the latter being drawn from them. The lengthened period during which the human ovum is capable of fecundation may possibly have been the cause why observation has yet proved nothing precise upon this point. On the other hand, it does not appear that any ordinary circumstances, such as physical and moral excitement of the sexual impulse, and the like, could easily induce a change in the character of the maturation of ova, and thus also in the possibility of fecundation. The fact is contradicted by all our experience with regard to menstruation, which proves, indeed, that that function can be, and actually is, sometimes accelerated or retarded ; but still only by the operation of causes which exert a far deeper influence than those alluded to. Of course, all that is known with respect to menstruation has its value in relation to this point, since it indicates the period of maturation of an ovum ; but the collected evidence with regard to that function shews an alteration, such as that mentioned above, to be something uncommon ; and all our latest information on the formation of corpora lutea proves only that the above-named influences can hardly possess such a power.

It is doubtless unnecessary to draw attention to the extraordinary importance attached to the discovery of a law which relates to one of the weightiest interests of mankind. Science, and society in general, are in the highest degree interested in it. On this account I most warmly desire that numerous further tests of its truth may not be wanting, and that it may not be opposed by antiquated deeply-rooted prejudices, which are sometimes regarded as experience. But whoever proceeds to the test should previously qualify himself for the task by much embryological

research, for he has to do with things which are not easily ascertained. The errors of centuries, not indifferent about the question, prove this. I hope to be able to meet well-grounded doubts, and will at once proceed to remove one such, which has already been frequently opposed to me. I have often been asked, "If conception be dependent on menstruation, and this again on the maturation of an ovum, how does it occur that women have frequently conceived who had never menstruated?" My reply is the simple statement, that although the secretion of blood be a normal, and the most striking phenomenon which accompanies the maturation of an ovum, yet it is by no means indissolubly connected with it. This is proved by the first glance at the animal kingdom, where this periodical maturation of the ova often occurs with, but more frequently without such a secretion. In the human female it is altogether a normal and important phenomenon, but by no means one which is essential,—it is only accidental. It may be absent, but mature ova may nevertheless be developed, and therefore also become impregnated. This will readily harmonize with what experience shews, that in general such women are also altogether free from any abnormal symptoms.

Again, many will no doubt question the law laid down, on the ground that it is scarcely conceivable that so important a circumstance should have escaped observation altogether, and especially that of anatomists and physicians, during their numerous disputes respecting the corpora lutea. To this I would reply, that it is quite true many striking points were long since known; as, for example, the dependence of fruitfulness in the female upon menstruation, the admission of the fact that conception takes place more readily immediately after menstruation, the reckoning of pregnancy according to the last appearance of that function, and the like. The reason why these observations did not lead to the full knowledge of the matter lies no doubt in the frequent recurrence of the menstrual period, or the maturation of an ovum, and possibility of a conception. If women menstruated only once or twice in a year, it would long since have been remarked that such was the only time when conception was possible; menstruation would long ago have been recognised as perfectly analogous to "the heat" in animals, even though the most essential element of it, namely, the maturation of ova, had not been discovered. The possibility of conception, however, is, on the one hand, so frequently repeated, in consequence of the menstrual period occurring every four weeks, that, in the absence of farther data, it was not easily possible to fix the point of time with which it was connected more accurately; and, on the other hand, menstruation passes over so frequently without its purpose, conception, being attained, that the attention must naturally have been directed more to that function than to the dependence of conception upon it. With regard to animals, it was the precisely opposite relation of

circumstances which proved the impediment to the correct perception of the analogy between "the heat" and menstruation. In them the period of heat either occurs so seldom, once or twice a year, or, when repeated more frequently, as in the cow, the sheep, the pig, &c. it is so interfered with by ordinary circumstances, such as our economical arrangements and the like, that the animals are either immediately impregnated, or the recurrence of the maturation of ova is obstructed and stopped by the secretion of milk. That an analogy, otherwise so striking, with one of our commonest domestic animals, the cow, should have remained for the most part unnoticed, is no doubt to be attributed to some of the above circumstances.

As regards the researches of anatomists and physiologists, we may remark, that they in the first place did actually make some correct observations; such, for instance, as those proving the formation of corpora lutea without coitus and conception. The fact that they did not interpret them correctly, nor develop them further, is an additional proof of the necessity for not only the corporeal eye and hand, but the intellectual eye also in our observations, and shows how necessarily our researches must be accompanied by an idea, by reflection, if they are to apprehend correctly even simple relations of things. But these ideas were not mature, they could not be so; and the eyes of men otherwise most distinguished could not therefore be enlightened by them. Lastly, however, the rare opportunity for making observations on this subject must certainly be allowed great weight. Such opportunities occur but very seldom after the usual mode of death, the result of disease. Menstruation, the maturation and extrusion of ova, is too intimately connected with the functions of health, not to be interrupted by almost all modes of death, and therefore it is that, on examination afterwards, nothing is found. It is only in the bodies of perfectly healthy persons, who have met a violent death, that we can hope to obtain the required circumstances for observation. And how many of such as die suddenly will meet with their deaths exactly at the period of menstruation? And again, how many of those who do, will fall into the hands of such as are competent to so minute an investigation? The most favourable opportunities for the purpose would be afforded in the cases of executed criminals, where the requisite facts with regard to dates and so on may be precisely fixed.

Giessen : February 1844.

[The remaining portion of the essay is chiefly devoted by Professor Bischoff to the establishment of his claims to priority in the discovery of the fact which it is written to establish. His great rivals are Raciborsky and Pouchet. Having omitted that portion, under the impression that it

would not be generally interesting, I feel myself bound to state that Professor Bischoff has so clearly and so fairly laid before his readers the facts of the case, that I think no one can have the slightest doubt of the merit being solely due to him. Others, as he says, may have brought forward *indirect* proofs, but he has furnished the *direct* ones. I beg, in concluding my task, to apologize to the readers of this journal for the rough, and sometimes almost literal style in which I have placed my friend's valuable essay before them ; I can only plead as an excuse, that my great object was to be faithful to my original, and that, had not the various divisions of the essay been prepared by me in great haste, and during a period of peculiar anxiety, I should gladly have devoted more time and labour to my subject.—*Translator.*]

Henrietta Street, Cavendish Square :
May 1845.

THE END.